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Annals of Science
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/tasc20

Cryptozoology, Archaeology and Palaeontology: Histories Near the High Table

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Version of record first published: 25 Jan 2010

To cite this article: Keynyn Brysse (2010): Cryptozoology, Archaeology and Palaeontology: Histories Near the High Table, Annals of Science, 67:4, 569-575

To link to this article: http://dx.doi.org/10.1080/00033790903243316

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Annals of Science,
Vol. 67, No. 4, October 2010, 569–575

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Essay Review

Cryptozoology, Archaeology and Palaeontology: Histories Near the High Table

Bernard Heuvelmans, *The Natural History of Hidden Animals*. Edited by Peter Gwynvay Hopkins. Translated by Kegan Paul. London: Kegan Paul Limited, 2007. xii + 145 pp. $120.00 (cloth). ISBN 978-0-7103-133-1.

Derek Turner, *Making Prehistory: Historical Science and the Scientific Realism Debate*. Cambridge: Cambridge University Press, 2007. xiv + 223 pp. $99.95. ISBN 978-0-521-87520-2.

Anne O'Connor, *Finding Time for the Old Stone Age: A History of Palaeolithic Archaeology and Quaternary Geology in Britain, 1860–1960*. Oxford: Oxford University Press, 2007. xxxix + 423 pp. ISBN 978-0-19-921547-8.

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In 1984, John Maynard Smith, a British geneticist and evolutionary biologist, welcomed palaeontologists back to the 'high table' of evolutionary biology after a forty-year absence precipitated, in his opinion, by the work of George G. Simpson, which made palaeontology consistent with, but no novel contribution to, evolutionary studies.[1] Others would trace this marginalization back further, to Charles Darwin himself, who dismissed the fossil record as too fragmentary to ever yield definitive evidence of evolution.[2] It was only with the rise of palaeobiology in the mid to late twentieth century that palaeontology truly carved out a unique niche for itself among other evolutionary disciplines.

For much of the twentieth century, archaeology was likewise refused a place at the high table of anthropology, taking a back seat to ethnography.[3] And cryptozoology, literally the study of hidden animals, has yet to be invited to anyone's high table, as it is still considered by many to be merely a pseudoscience (think Bigfoot and the Loch

[1] John Maynard Smith, 'Palaeontology at the High Table', *Nature*, 309, no. 5967 (31 May 1984): 401–2.
[2] David Sepkoski and Michael Ruse, eds, *The Paleobiological Revolution: Essays on the Growth of Modern Palaeontology* (Chicago, 2009).
[3] R. Lee Lyman, 'Archaeology's Quest for a Seat at the High Table of Anthropology', *Journal of Anthropological Archaeology*, 26 (2007), 133–49.

Annals of Science ISSN 0003-3790 print/ISSN 1464-505X online © 2010 The Author
http://www.tandf.co.uk/journals
DOI: 10.1080/00033790903243316

Ness monster). Three recent books discuss the history (and in one case, the philosophy) of these marginalized or formerly marginalized disciplines, and they are a welcome addition to the history of science literature.

Bernard Heuvelmans's *The Natural History of Hidden Animals* is at once a history of cryptozoology by the man who named the field, an explanation of what his discipline is and is not, and an impassioned plea for other scientists to recognize cryptozoology as a legitimate field of truly scientific inquiry. This book, I think, succeeds more in its first two aims than in its last. Cryptozoology is the study of animals whose existence has not yet been proven and which are presently known only through the testimony of witnesses or by circumstantial evidence. Heuvelmans makes an excellent case for the existence of as-yet-undescribed animals, and the relatively recent scientific recognition of living cryptids including the coelacanth, the kraken or giant squid and the giant octopus are excellent reminders that not all corners of our Earth, especially its oceans, have been fully explored.

The designation of cryptozoology as a unique scientific discipline distinct from zoology proper is, however, more problematic. Heuvelmans lambasts critics of cryptozoology (including the aforementioned Simpson, who, if Maynard Smith and Heuvelmans are right in their accusations, has thus compromised not just one scientific discipline but two) for thinking cryptozoologists are only interested in monsters. Such accusations may be unfair, but if Heuvelmans's own definition is used instead, then every zoologist who ever discovers a new species of animal, however small, unexciting, and similar to known species, is doing cryptozoology, whether he or she knows it or not. This definition is so broad as to be virtually useless.

Perhaps cryptozoology is more a function of a scientist's attitude, rather than the particular creatures he or she studies; for Heuvelmans calls repeatedly for a return of the true form of scientific scepticism, which means questioning one's own assumptions and preconceived notions, not just questioning fragmentary and subjective reports of new creatures. This advice, as far as it goes, seems sound. The book also includes dozens of black-and-white photographs and drawings of cryptozoologists and their fantastic beasts, mythical or formerly believed so, which are themselves fascinating. I think this book will function best as a sort of encyclopaedia for historians interested to know which figures of the past wrote about which fantastic creatures; Chapter 6 especially is a compendium of such reports.

Next we turn to a history of relative dating in archaeology, focusing chiefly on the work of English scientists. Anne O'Connor's *Finding Time for the Old Stone Age: A History of Palaeolithic Archaeology and Quaternary Geology in Britain, 1860–1960* explores the interconnections and conflicts among the different methods of telling Palaeolithic time: methods which included chronological sequences based on stone tools, geological evidence (including glacial and river deposits), and palaeontological (i.e. animal fossil) specimens. (Another source of evidence which might be used to date tools and tool-makers, the skeletal remains of Palaeolithic peoples themselves, is occasionally mentioned but generally not integrated into the discussion.) A general pattern of increasing complexity and age emerged through this period, as more and more evidence indicated that there had not been a single glacial epoch that covered most of Europe in ice, but several, and that human tool-makers had lived between (if not before) these glaciations, and not only after, as Palaeolithic researchers of the early nineteenth century had believed.

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The Palaeolithic, for those not in the know, is the earlier part of the Stone Age, characterized by a variety of stone tools (including core tools or hand axes, and a variety of flake tools such as knives, scrapers, and spear points) which can be found in gravel and dirt deposits at various locations around the world. Today the Palaeolithic is thought to span a chunk of time stretching from 2.6 million to 10,000 years ago, and as such encompasses the entire Pleistocene Epoch (1.8 million to 10,000 years ago) as well as the last quarter of the Pliocene Epoch (5.33 – 1.8 million years ago) of the Neogene Period (or, according to traditional classification, the Tertiary Period) of the Cenozoic Era. I emphasize this because nowhere in her book does O'Connor give the absolute age of the time periods under discussion, nor a geological time scale to help readers understand terms like 'Quaternary', 'Tertiary', or 'Pliocene', and their relationship to other, not-exactly-parallel time divisions, like 'Palaeolithic', 'Neolithic', etc. O'Connor's avoidance of whiggism and her dedication to reviving the context and conclusions of her historical actors are laudable, but without some frame of reference in which to place these and other terms, chronology occasionally becomes as confusing for the reader as it did for the Palaeolithic researchers themselves.

The first two chapters outline those methods of dating which do not rely on an analysis of stone tools: the fossil record (specifically, the changing record of large mammals, including mammoths, woolly rhinos and giant reindeer) and the geological record (including the boulder clays and other unusual deposits which were first taken as evidence of the Biblical flood, and later interpreted as the results of one or several major glaciations). Chapters 3–6 describe the stone tools which are the chief focus of the book, and which had to be interpreted and arranged in a sequence consistent with the chronologies derived from the fossil and geological evidence. This section, the backbone of the book, is a fascinating read. In Chapter 5, the reader is introduced to 'eoliths', extremely old artefacts which might be the very primitive tools of the earliest hominids, or which might just be rocks, broken by natural processes and only coincidentally resembling deliberately worked tools. This chapter serves as a reminder that, all too often, we see what we want to see: Palaeolithic researchers looking for the oldest tools of the most primitive men, fully expecting them to be crude compared to later examples of much finer craftsmanship, found exactly what they expected in these eoliths (which, today, are generally dismissed as natural objects).

One of the greatest strengths of this book is its wealth of archival source material, chiefly in the form of personal correspondence. Chapter 6 includes a fascinating tale of a visit by two leading French geologists, Henri Breuil and Marcellin Boule, to the British scientists E. Ray Lankester and J. Reid Moir in Ipswich, in September of 1912. The French scientists were not convinced that the 'pre-palaeoliths' (eoliths by another name) that their English colleagues so carefully showed them were genuine stone tools. Although Lankester's professed intention had been to 'bring Boule & Breuil face to face with *the facts* and, since they are intelligent men and experts, let them draw their own conclusions', he was not above rigging his presentation of 'the facts' by dictating exactly which specimens should be presented to the visitors, and especially which of his fellow English geologists were and were not allowed to be present during the visit, lest they say anything that distracted the Frenchmen from 'correctly' interpreting the specimens (p. 192). After the unsuccessful visit, Boule and Breuil were 'intelligent men and experts' no longer: Lankester wrote to Moir that 'A matter of inference based on tangible evidence, can not be dealt with by the mere

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opinion of two hasty foreigners who deliberately chose not to consider the specimens & other facts which are opposed to their fanciful notions' (p. 193). This is a fascinating example of how one scientist saved his interpretation in the face of its rejection by his scientific peers. (It shows, also, that the true scepticism Heuvelmans sought was sometimes a rare beast not only in cryptozoology but in other disciplines as well.)

Finally, Chapters 7–10 detail the struggles of a large cast of characters as they tried to fit the growing mounds of archaeological, geological, and palaeontological evidence — now from Africa as well as Europe — into a coherent chronology of ancient hominid tools and cultures. A nine-page list of 'Dramatis Personae' at the beginning of the book is a very helpful device for keeping these characters — some of whom appear in multiple chapters — straight.

One area of the book that might have been expanded is a discussion of general stratigraphic and archaeological principles. Readers with an armchair knowledge of geology or palaeontology may be surprised to learn that when dealing with very recent sediments, higher layers are not always younger (for example, since rivers erode down through their beds over time, a lower river terrace is often younger than a higher one — although Steno's Principle of Superposition still holds within each individual terrace). A tree diagram depicting the genealogical relationships between tool industries, according to one anthropologist of the 1920s, reveals another mystery that would benefit from exposition (p. 82). Although this diagram shows time increasing up the vertical axis, as in typical evolutionary tree diagrams, the tree itself is inverted: instead of branching outward from a common trunk, the branches on this tree join together over time. In explanation O'Connor says only that 'Genealogical trees have a long history and were easily adapted for palaeoanthropological purposes'; she does not explain why this adaptation has included turning the genealogical tree upside-down, nor what the joining together of branches might represent (my guess is cultural migration) (p. 282).

Unfortunately, there is little historical analysis throughout the book; the author saves it all for the fifteen-page conclusion. In this conclusion, O'Connor explains:

> The aim of this book has not been to build general models of scientific knowledge .... Instead, these chapters have looked closer at some of the turbulent undercurrents of research, reviving the roles and lives of forgotten researchers through snatches of biography, exploring what they thought about the age and character of ancient stone tools, why they came to these conclusions, and why some were more successful in winning support than others. On this scale of analysis, general historical patterns become scattered through the specific personal, social, and local contexts of individual researchers. (p. 357)

O'Connor is successful in her stated aim — perhaps a bit too successful. In sacrificing a view of the forest for a focus on individual trees, as the cliché goes, any general historical patterns do indeed become scattered and lost. A bit more general historical analysis would in fact be welcome, just to tie together the many detailed threads of the individual researchers into some coherent whole. Nevertheless, O'Connor has succeeded in providing a rich description of thoughts, chronologies, and classifications of Old Stone Age tools between 1860 and 1960. Anyone wishing to explore deeper historiographic or philosophical questions about this time period and subject will find this book a rich source of raw material.

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Derek Turner's book *Making Prehistory: Historical Science and the Scientific Realism Debate* was chiefly written to address a serious problem in current philosophy of science, which it does admirably: the failure of many philosophers of science to take historical, and not just experimental, sciences into account while constructing and evaluating their theories for or against scientific realism. In addressing this problem, instead of adopting the more traditional nomothetic vs. ideographic dichotomy of experimental vs. historical sciences — one which, Turner suggests, buys into the hierarchical view in which experimental sciences such as physics and chemistry are 'harder' and 'better' than historical sciences like geology and palaeobiology — Turner suggests a new dichotomy.

This new dichotomy distinguishes the kinds of unobservables one finds in historical sciences from those found in experimental sciences. According to Turner, historical unobservables suffer from two asymmetries with respect to experimental unobservables: (1) the asymmetry of manipulability, which recognizes that many unseen entities of the present can be experimented upon, but the entities of the past can never be changed or manipulated; and (2) the role asymmetry of background theories, which notes that background theories in the experimental sciences often suggest ways to find or manufacture new evidence, whereas those in the historical sciences usually lead scientists to expect that the evidence they seek has been destroyed by the ravages of time. While the first asymmetry seems self-evident, the second does not, and I think Turner has not succeeded in proving it as a general principle. One could point to at least some background theories in experimental sciences that suggest one might not find all the evidence one seeks (the Heisenberg uncertainty principle comes immediately to mind). Similarly, one can also think of some background theories in historical sciences that suggest ways to find, not lose, fossil evidence (principles of sedimentation in quiet waters along shorelines, perhaps). Turner has not demonstrated that the *majority* of historical background theories play a 'dampening role' while the majority of experimental background theories play an 'enlarging role', to use his own terms (p. 24). This is not a fatal flaw, however, and Turner's main argument — that the historical sciences have something unique and important to say about scientific realism — still holds.

The remainder of the book is devoted to two general tasks. The first task consists in showing how these asymmetries put the historical sciences at an epistemic disadvantage compared to the experimental sciences. For example, in Chapter 4, Turner shows that the use of observable analogues is just as fraught with potential error in the case of past unobservables as in the case of present but tiny unobservables; while in Chapter 5, he demonstrates that novel predictive success is even harder — though not impossible — to come by in historical vs. empirical sciences. This chapter also includes a very interesting discussion of some recent methods palaeobiologists have adopted to try to deal with this problem, including quantitative analyses and computer modelling. Other historians and philosophers of science — notably David Sepkoski — have also studied the 'quantitative revolution in palaeobiology', and I think Turner's take on the motivations behind this revolution can and hopefully will serve as a fruitful basis for further study.[4]

[4] David Sepkoski, 'Stephen Jay Gould, Jack Sepkoski, and the "Quantitative Revolution" in American Paleobiology', *Journal of the History of Biology*, 38 (2005), 209–37.

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The second task, which is the chief aim and greatest success of the book, is an examination of current anti-realist theories from a perspective which focuses — for essentially the first time — on the historical sciences. Turner convincingly demonstrates that considering only the experimental sciences leads to false confidence in one currently popular anti-realist theory, Bas van Fraassen's constructive empiricism. van Fraassen argues that scientists can have no genuine knowledge whatsoever of unobservables. This may not seem too radical when considering only the experimental sciences, which describe far more observables than unobservables, but when one takes the historical sciences into account, as Turner does, constructive empiricism forces one to either assert (ludicrously) that past events and objects are observable after all, or to commit to an 'unacceptably radical skepticism' about historical facts and events (p. 204).

Turner also examines Arthur Fine's natural ontological attitude from a historical science perspective, and finds it much more congenial. Indeed, Turner more or less adopts Fine's theory, calling his own position the natural historical attitude. This attitude, as Turner explains in Chapter 7, combines epistemic realism with metaphysical agnosticism: we should believe we have genuine knowledge of the past, but do not and cannot know for certain if that past is discovered (the realist position) or created (the constructivist position).

Turner writes clearly, concisely, and wittily, and his lucid explanations of complex and potentially confusing philosophical schools and theories are particularly welcome. Readers without much prior philosophical or scientific knowledge should have little difficulty following his explanations and examples, so this book would make an excellent textbook for a junior-level course in philosophy of science generally, or realism specifically (especially as a counterpoint to other texts discussing only the philosophy of the physical sciences).

Readers with a background in the earth sciences may have a few minor concerns, however. While the book benefits greatly from actual case studies drawn from past and current debates in geology and palaeobiology, their credibility is slightly diminished by a series of typographical errors, including the frequent (but not consistent) failure to capitalize terms such as 'Neoproterozoic', and the unfortunate use of 'therapod' instead of the correct 'theropod'. Also, some vertebrate palaeontologists might be dismayed by an example in Chapter 2 in which Turner asserts, following Carol Cleland, that 'the presence of iridium and shocked quartz at the Cretaceous–Tertiary boundary, not to mention the Chicxulub crater in Central America, are smoking guns for the Alvarez hypothesis that an asteroid impact triggered the extinction of the dinosaurs'. Turner argues that the impact hypothesis wins because 'none of the other potential explanations of the Cretaceous–Tertiary mass extinction imply the existence of a crater', and presumably, since a crater was found, the mass extinction hypothesis that predicts it is the best one (pp. 39–40).

What Turner and Cleland fail to appreciate is that while the iridium, shocked quartz and crater are indeed smoking guns proving an impact took place, they are not also therefore smoking guns linking the impact to the K-T mass extinction. As dinosaur palaeontologist Peter Dodson noted in 1992, supporters of the impact hypothesis 'may have a smoking gun, but some victims died of stab wounds'.[5] As

[5] Peter Dodson, quoted in Richard A. Kerr, 'Did an Asteroid Leave its Mark in Montana Bones?' *Science*, 256, no. 5062 (5 June 1992), 1395.

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Turner notes elsewhere in the book (Chapter 8) but fails to realise here, a successful hypothesis should be consilient: that is, it wins over its rivals by successfully accounting for *all* of the relevant traces and background knowledge. As vertebrate palaeontologists have repeatedly tried to point out, the impact debate is a prime example of a case in which relevant and important evidence and background knowledge regarding extinction (including the fossil records of specific K-T extinction victims, as well as geological evidence of other mass extinctions without impacts, and other impacts without mass extinctions) have been excluded by some parties, thus biasing the evaluation of competing hypotheses.

These caveats aside, readers with less stake in the mass extinction debates specifically, and a more general interest in philosophy and the historical sciences, will and should applaud this rare effort to construct a philosophy of science that is not actually and merely a philosophy of physics. It is exciting to see historians and philosophers of science writing about these marginalized or formerly marginalized disciplines, and indeed, all three authors — Turner, O'Connor, and Heuvelmans — should be applauded for their contributions.

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